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PYREXIA:
A RETROSPECT, A REVIEW, AND A FORECAST.

Inaugural Address

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BY

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GENTLEMEN,—

I have first to thank you very sincerely for the great honour you have done me in calling me to this Presidential Chair. The pleasure and satisfaction which cannot fail to attend the honour conferred would be unalloyed were it not that, in obeying your call, introspection and a reckoning up of deficiencies are involved. Whatever the shortcomings, I rely on your indulgence; and, assured that this will not be withheld, I place such service as I may be able to give unreservedly at your disposal, and assume the high responsibilities of the office to which you have elected me.

The next duty devolving upon me is to read you a Presidential Address. Nor is this, by any means, the lightest of the duties of my office. In selecting a topic upon which to discourse to you, I have been guided entirely by personal considerations. The topic of this address is one that has interested me; the personal interest has determined the selection, and this interest alone. It was selected simply because it happened to be the thing that interested me at the time. For this, perhaps, I ought to offer an apology. But I trust, and I would fain believe, that what has interested me may also interest you; inasmuch as the topic and the point of special interest in it concerns a subject that is always before us as practitioners, and a subject upon which we hunger and thirst, so to say, for fresh knowledge and for newer lights—Fever, to wit.

It was at first my intention to speak of Fever from another point of view. But, as I was busy working up the historical side of my then subject, in that store of old English classics—the Library of the Royal Infirmary—I was drawn to take a different course, because a new interest was awakened. About that time my attention had been directed to the fact that a remarkable application of the mechanical theory of heat to molecular changes, and especially to the interpretation of chemical changes, which had been developing during the past ten years, had made such progress that the new views were commonly taught in our higher schools of chemistry, though they have not yet found their way into the text-books. I refer to the views of chemical combination and disassociation which, discarding the now defunct "affinities" and "repulsions" between atoms, account for chemical change and chemical union on mechanical principles, and deduce

the facts of chemical change and chemical union from the law of Dissipation of Energy—itself a deduction from the law of Conservation of Energy.

These new views, developing outside the limits of practical Medicine, seemed to me destined to extend considerably our knowledge of the fever-process. Looking at fever-pathology in the light of this new application of thermo-chemical views, my attention became fixed upon the central and essential fact of Fever—increase of heat with elevation of temperature. I learned that what is to us *the* essential fact, was also accounted *an* essential fact in Fever far back in the history of Medicine. I learned that the accurate observers and acute reasoners of times long past had been struggling to give a due significance and a true explanation to this fact—and, indeed, that they did give such explanations of the fact as the knowledge of the times about the nature of heat and its connections with matter permitted. And I also learned that we are yet a long way off from knowing all the truth about the essential phenomena of Fever—although it may not be presumptuous to exclaim that we see the outline of the goal looming in the distance.

Then, my thoughts took shape in an aphorism: The knowledge of the Present is the outcome of the work of the Past; and the work of the Present is the basis of the knowledge of the Future. In this aphorism the key-note of my address is sounded.

Herein is implied a retrospect; to the better understanding of our present position with regard to the pathology of the fever-process. I invite you to this retrospect. Not for the mere purpose of tracing the development of our knowledge through the observations and reasonings that have led to it; nor, alone, for the sake of

an interest that belongs to all history. There is a point of special interest in the retrospect which has not hitherto, so far as I know, been brought into view; and this it is my object to unfold. It will appear, I think, that the notions held in the past as to the pathology of the febrile state have depended upon and progressed with the progression of knowledge concerning the nature of Heat. And the aphorism implies a review; to which also I invite your attention. The knowledge and the work of what we may call the Present—of this present half-century—is in continuity with the knowledge and the work that went before it; and both reflect the notions prevailing in the successive epochs about the nature of Heat. And, further, the continuity of the work and the knowledge of the past with the work and knowledge of the present, must be extended to the future. For, already, the progress of thermo-chemistry has made it clear that the knowledge of the future concerning fever pathology will be the outcome of work now going on, yet only partially developed, in respect of the relations between matter and energy, and energy in the form of Heat. Our aphorism suggests a forecast, to include this future progress; and thus, a welding into a continuous chain of consistent and progressive knowledge the ideas obtained inductively by mechanical and analytical sciences, but drawn off and applied deductively by Medicine in one of her most serious efforts to probe the mysteries of disease. Wherefore, the title of this address.

Before taking up these several points in their order, it will be proper to explain what I mean by the Pyrexial state, and the essential fact of that state. I mean by the Pyrexial state that which *is* fever, and common to *all fevers*. Fever in the abstract, as some would have us

believe. "That *abstract* condition which is common to all so-called 'febrile disorders,' and the presence of which gives them their claim to that designation." (Bristowe.) But we must not recognise the use of any such term as "abstract condition" in our practical science, Medicine—if, indeed, the term means anything at all. The Pyrexial state is a fact—a grouping of facts, if you will; it is a state of things that can be experienced and felt: it is open to observation, and its phenomena can be described as matters of fact.

My conception of the Pyrexial state—or, simply, Pyrexia—is this: Pyrexia is a disease-process, set agoing by a great variety of circumstances. It has its own characters, by which we can identify it, and recognise its existence as a factor in many different diseases; in every fever we note certain characters which are repeated, with some variation of detail, in all fevers. These characters define the state. The characters depend on deeper facts, some of which we know: by means of these deeper or essential facts we explain the characters; and by means of these deeper facts the characters of Pyrexia have been generalised in the single fact—elevation of temperature. The deeper facts have also been explained by a large induction and by the application of deductions from chemistry and physics; we know now that the essential fact, the increase of heat (and consequent elevation of temperature), has its origin—at least, in great part—in increased tissue metamorphosis. And this state, Pyrexia, has its own modes of evolution; the characters present definite variations during the working-out of the process. Hence, by study of the evolution of Pyrexia, we are able to construct "types" of fever—as instanced in the hectic and typhoid modes. Whatever we may find in examples

of fever more than this—whether characters added, or more explanation needed, or special modes of evolution,—we can always find in these examples the characters, the essential facts and the evolution of the Pyrexial state. In actual examples of a fever, the variations in characters and in details of evolution are apt to be voluminous and complicated; and these are the things that create the interest, and call up the experience, and call out the skill of the practitioner. But in all circumstances and in all vicissitudes, the central and essential fact of Pyrexia remains—viz., increase of heat,—and the characters, modified though they be, remain as witnesses to the presence of that fact.

Now, for the Retrospect. We start from that point which, by common consent, for close upon twenty-four centuries has been held to be the starting-point of positive Medicine—the time of Hippocrates, B.C. 460. It is a common infirmity of the modern medical mind—dazzled, perhaps, by the brilliant lights of modern science—to regard all times anterior to our own as dark and barren of truth, and all medical knowledge and speculation of those times as one gigantic mistake. Most of those who review the history of medical thought and science in times prior to this century, seem rather to be influenced by pity and contempt, and to be actuated by the desire to trace the progress of error instead of the progress of truth. Many facts once supposed to be true, many theories that have caused revolutions in science, have, even after many generations, been refuted, deposed, and dismissed. But many such have also contained something that was sound and permanent. In reading the history of Medicine we must not overlook the truth in our sweeping denunciation of the error in which it may lay embedded.

Hippocrates, as is well known, acknowledged an increase of heat in fevers: he speaks of it as an increase of the *calidum innatum;* and this innate or natural heat of the body he associated with the principle of life. Here is what he says: "What we call heat, that seems to me to be immortal, and to understand, and see, and hear, and know all things whatsoever both present and future." To Hippocrates, this increased *hotness—not heat*—was, as it seems to me, only a symptom; he ventured on no pathology; his business was to observe the superficial characters of disease. He had, indeed, no data on which to found a pathology of fever; he had no theory of heat with which to connect his facts. It required no genius to do what Hippocrates did: to recognise the warmth of the normal living body and the hotness of the fevered body. But he was wise enough to advance no further than his facts and his knowledge allowed; so he said, in effect: What *we call* hotness of the body, whether in health or in disease, is part and parcel of our life.

The influence of the Hippocratic methods and sayings during the succeeding centuries is matter of history. Greek followed Greek along the road that Hippocrates had opened. Great advances in the knowledge of anatomy and disease were made, and the Greeks carried their knowledge into Egypt, where was established the celebrated School of Medicine of Alexandria—a school founded by Ptolemy the First, the brother-in-law of the Macedonian king, Alexander. Then comes the school of the "Methodics," of which school Celsus was one of the brightest ornaments. Celsus, nearly 500 years after Hippocrates, gathered up in his writings all that was Hippocratic in the work of his predecessors. But still there is no trace of a pathology of fever. In Galen's

works we first find a trace of a pathology of fever. Galen wrote about 200 years after the Christian era. The præternatural heat of fever was caused, said Galen, by a species of putridity set up in the humours of the body previous to the fever. This conclusion appears to have been reached by connecting the changes observed in stagnant liquids when they are exposed to heat, yet not evaporated, with the appearances in the urine of fevered persons. Here it may be mentioned that this conception of putridity in connection with fever is found in works published in the last century—*e.g.*, Huxham, *On Fever*. Galen also developed the doctrine of "Concoction," a doctrine to be traced in the writings of Hippocrates. This doctrine, applied to fever, asserted that the morbid matters in the humours of the body underwent a change, assisted by the application of heat, which fitted them to be separated and thrown off from the body. This, however, concerns the therapeutics rather than the pathology of fever.

There is no evidence that Galen was an original observer, like Hippocrates; he himself professed only to select and advocate what was true and valuable in the work and opinions of his predecessors. Galen's name and writings are, however, important in the history of Medicine for the influence they had many generations later. Little was done after Galen's time, and up to the revival of learning in the 15th century, to advance the knowledge of Medicine. Here and there a name shines out—*e.g.*, Ætius, Alexander the Thrallian, Paulus Ægineta (7th century), Demetrius, &c.—but what was done seems to have been chiefly in the direction of collecting symptoms and signs of disease, explaining these by theories as to the constitution and changes in the fluids of the body, and devising modes of cure on abstract principles.

During the period when, consequent on intolerance and persecution, men found almost full occupation in guarding and preserving what they could of the knowledge and learning already acquired, we could expect little or no progress in the science, or even in the art of Medicine. As the Dark Ages began to disperse, from Persia and Arabia came back to Europe the medical knowledge and science that then survived; and this was chiefly deposited in the revival-school of Salernum, in Italy. Here the works of the old Greek physicians, which had been translated into Arabic (after the Nestorians, flying from persecution, had settled in Arabia and Edessa in Persia), were re-translated into Latin from the Arabic. And thus Medicine actually begins over again with Hippocrates and Galen fifteen centuries after their time. Cullen says that it was the system and doctrines of Galen the physicians of the 15th century became acquainted with; and that during the 16th century physicians were almost solely employed in explaining and confirming this system and these doctrines. And he accounts for it in this way: that on the revival of learning certain Greek physicians came to Italy, bringing with them their books. The Europeans, studying these books, especially Galen's works, found that they contained almost all the medical knowledge which had been, up to that time, attributed to the Arabians. So the Europeans applied themselves to the study of Galen, as the fountain of medical knowledge, and thus Galenical doctrines became prevalent over Europe. Indeed, down to the middle of the 17th century, when Harvey discovered the circulation of the blood, there was no pathology. Physicians had been busy systematising the superficial appearances and signs of disease; and as to

causes, they regarded only those that were proximate. What explanations they gave of the phenomena of disease turned entirely upon the constitution and state of the fluids of the body—a humoral pathology, if the term pathology be applicable at all.

It is surprising that this was so; yet so it was. The notions as to chemistry and natural philosophy had been, up to this time, chiefly applied to therapeutics; although Paracelsus and Van Helmont had suggested explanations of disease—including fever—in accordance with the then chemical doctrines, and the materies morbi of fever was regarded as a mixture of sulphur and saltpetre. But then, this was alchemy, and not chemistry. The iatrochemical school of physicians had also been founded by Sylvius, a sect who attributed disease to the effects of chemical relations of the fluids.* If we turn to other branches of knowledge, we find the same stagnation that we observe in Medicine during this period. Thus, for Mechanics—a science upon whose progress and development Medicine has depended in no small degree, especially in regard to the nature of heat—from the time of Archimedes, B.C. 212, to the time of Stevinus and Galileo, A.D. 1600 (about), no single advance was made (Whewell).

It comes to this, then, as the result of the first part of our retrospect, from the time of Galen to the 17th century—that, whilst a great store of imperishable "clinical" knowledge was amassed, no progress in pathology was made. Nor was such progress possible, owing to the peculiar circumstances of the times. And in particular, no progress with fever-pathology was made, or was pos-

* But, in truth, chemistry can hardly be said to have existed as a science until the discovery of oxygen by Lavoisier in 1783, though preceded during the 18th century by much good work which paved the way for the further advance.

sible, owing to the imperfect development of those sciences upon which the explanation of the nature of heat depends.

Now comes a period—the 17th century and first half of the 18th century—in which advance in all departments of the physical sciences was rapid and decisive. Francis Bacon introduced the method of induction just at the beginning of the 17th century. Harvey made known the circulation of the blood in 1619, before Bacon died. Galileo propounded the first law of motion in 1638. Descartes and Boyle lived in this century, and Newton followed towards the close of the century. During this century, also, great advances were made in Anatomy and Physiology. The great intellectual activity, and the great and solid advances made in physical science, during the 17th century, were not without effect on the progress of Medicine. And this progress was, as might be expected, on the mechanical side. A few instances may be mentioned. The laws of the motions of fluids came to be applied in pathology. The first conception of heat as motion was introduced. The thermometer was invented in this century. The effect of the discoveries and scientific ideas of this age on Medicine was not only to introduce mechanical notions into Medicine, and to create the iatro-mathematical or iatro-mechanical school, but to induce a more exact method of reasoning upon facts obtained by experiment. Wherefore, the estimation of weight, the calculation of motion, and the measurement of heat, were amongst the results of the work of this age.

Time will not permit a closer scrutiny of this most interesting and instructive period in the history of Inductive Science. Let me therefore illustrate our thesis by reference to the writings of a man who remodelled the

Galenic system in accordance with the mechanical views of the 17th century, and who exerted a great influence upon the further progress of Medicine—Hermann Boerhaave, who lived and taught between 1668 and 1738. And it will be convenient to take him in conjunction with one of his pupils—Van Swieten—who, in his well-known *Commentaries on the Aphorisms of Boerhaave*, gives an insight into the development and practical outcome of Boerhaave's teaching down to the close of the 18th century.

Be it understood, there was as yet no universally accepted scientific conception of the nature of heat. Heat was, for the most part, regarded as a kind of motion in matter; the motion was produced by fire, and fire was caused by a special igneous matter. Bacon, however, said: "Heat is produced by the motion of attrition, *without any preceding heat.*" And at the time of Boerhaave it was known that heat caused solids to become fluid. Hooke says: "Heat being nothing else but a very brisk and vehement agitation of all the parts of a body, the parts of a body are thereby made so loose from one another that they easily move any way and become fluid."

Such were the views as to heat prevalent among the natural philosophers of the 17th century. In vol. v. of the *Commentaries on the Aphorisms of Boerhaave*, by Gerald Van Swieten—of an old English edition published in 1757—we find an account of *fever*, as distinct from fevers, which may be thus condensed: The nature of fever, being latent or concealed, must be searched into with great caution. In the search "we are only to consider the appearances of the fever present in the body," to the end that we may understand the proximate cause of fever. But the great number of appearances, or symptoms, at-

tending a fever may lead into error. Wherefore we must select such "symptoms or appearances" as *always* attend *every* fever. And then, from these symptoms, we must thence search into the individual nature of a fever. Boerhaave took *all* the symptoms that had, up to his time, been observed in fevers. He then "blotted out" all those symptoms that occurred only in particular fevers, and retained those which, by the common consent of authors and his own observation, were present in every fever. Then, comparing these, he tried to get at the individual nature of a fever. He was surprised to find three symptoms only as the residue; viz., a shivering, a quick pulse, and heat, varying in degree at different times of the fever. He says "the shivering attends when the fever begins, and is then followed by a heat, gradually increasing, and always the most intense (cæteris paribus) in the height of the fever, decreasing again afterwards by degrees with the fever itself." He then says that the quick pulse is the only symptom present through the whole fever from beginning to end. Therefore, "whatever the physician knows of a fever is derived from the quickness of the pulse." The proximate cause of the velocity or quickness of the pulse is likewise the proximate cause of the fever. Now, this cause is a quicker contraction of the heart, together with increased resistance in the capillary vessels. Elsewhere, in vol. vi. of the same edition of the *Commentaries*, the heat in fevers is discussed separately. The signs by which to distinguish the external heat from the internal heat are first set out. The external heat is to be measured by the thermometer; and rules for using the instrument are given, exactly corresponding to the rules by which we proceed at the present day. The internal heat is known by the redness of the urine.

A very pretty bit of reasoning on the then known facts connects the cause of the increased heat in fevers with the appearances of the urine. But this febrile heat requires a greater quantity of fire; and the greater quantity of fire arises from a more violent attrition of the fluid parts against each other and against the sides of their containing vessels. An objection, raised by one, Schelhammer, is now considered: "The most rapid rivers run swiftly over immense rocks for whole ages together; yet they never become warm, much less do they grow hot;" there must be another principle than motion by which the blood grows hot. Van Swieten answers: Heat may be excited in fluids by attrition, if they are elastic; but otherwise with difficulty. "Yet milk, being swiftly churned, grows warm." Fluids, not elastic, if urged with great force through very small vessels, will grow warm by attrition, especially if the vessels are elastic. Now, Leuwenhoech had already shown that the blood contained a vast quantity of elastic spherical particles—the red globules. In the minute arteries the blood, driven with great swiftness, parts with its thinner portions, which escape through the lateral secretory vessels, and then hardly anything but red globules remains. Thence there comes a violent attrition between these elastic bodies and the sides of an elastic conical vessel, from which attrition heat arises.

The motion of the blood, derived from the increased action of the heart, is then discussed in a philosophical manner in accordance with the laws of motion (so far as then developed). All this is set out in detail with great clearness; even the mechanical alterations in the density of the blood and in the size of the vessels are set forth minutely and explained, with frequent reference to experi-

ment, and the motions are estimated in terms of the law that "the powers in moving bodies are in a ratio compounded of their masses (quantity of matter) and the square of their velocity." Herein is no vain hypothesis, and a twisting of facts to make them support it; but a calm, patient observation of facts—a comparison of the facts with each other and with other facts in outlying sciences, and a reasoning upon the facts after the methods of the Baconian philosophy. Here, indeed, is truth enshrined in error; work of which present knowledge is the outcome; strong, vigorous and exact induction, upon which much of our later deduction rests.

We think those have erred who have supposed that this was a retrograde step in the explanation of the fever-process. On the contrary, it was an important step forwards. It distinctly limited the range of enquiry by applying mechanical principles to the solution of a pathological problem. It *can* be no more than an enquiry into the explanation of symptoms, not—in spite of the word "essence"—an explanation of what we understand as "essential facts." *Signum pathognomicum* are the words used by Van Swieten. Indeed, he says, "There is only this one way of discovering the nature of diseases; viz., by considering each of the *symptoms or appearances* of the disease by itself, then by comparing them with each other, and with what usually happens in a state of health, and from thence, by just reason, to discover the proximate cause of the disease. For thus, by a constant observation, we discover in every fever that the velocity of the pulse is increased, and that therefore the heart contracts more frequently or swiftly; and thence again, that those causes from whence the contraction of the heart results are increased: but in what manner those

causes act which excite the heart to a quicker contraction are hitherto concealed from all of us. For all that we know of the nature of a fever, we discover only by its inseparable effects and appearances. Therefore, the three common or inseparable *symptoms* are to be considered in order." Confusion had been made between the *nature*—*i.e.* essential facts—of fever, and the essential, or diagnostically important, *signs* of fever. The old authors, as understood by one another, were not in this confusion at all.

During Boerhaave's lifetime, Frederick Hoffmann, of the University of Halle, had propounded a system of Medicine founded on a theory of nervous influence. This introduction of nerve agency into pathological notions had no real influence on the further progress of pathology, but it gave rise to a deluge of useless systems. Von Haller, following Hoffmann, established a theory of irritability and sensibility, and this led to a long train of neuro-pathological theories of fever. We might dismiss these as wholly unworthy of examination, were it not that some prominent names in the history of Medicine have been associated with the doctrines.

Cullen, who published his *First Lines in the Practice of Physic* in November, 1783—and who spoke with great authority from the Professorial Chair in Edinburgh—adopted and taught a view of the fever-process founded on the theories of Hoffmann and Haller. He says: "The idea of fever, then, may be, that a spasm of the extreme vessels, however induced, proves an irritation to the heart and arteries (whereby their action is increased), and that this continues until the spasm is relaxed or overcome." The spasm he conceived to be a part of the operation of the *vis medicatrix naturæ*.

The view we are inclined to take of these doctrines is this: that the physiology of the nervous system is such in its nature as to provoke to great speculative application; and when this subject broke fresh in upon the Medicine of that age, it found men in that temper to indulge in unlimited speculation. Chemistry was at a standstill; mechanics had been applied until men had gone as far as they could with it. What wonder that the mysterious and subtle phenomena of nervous action and electricity should have been eagerly seized upon as containing the true key to all medical problems! The attempt was even made to explain fever by an application of the doctrine of polarity!

It was reserved for Chemistry to make the next great step in advance.

Towards the close of the 18th century, the work of Black, and Cavendish (who first decomposed water), and Priestley and Lavoisier, made a fresh and most important epoch in the history of chemistry. Priestley and Lavoisier separately discovered oxygen in 1783. This discovery soon gave birth to the theory of combustion known as *oxidation*. Lavoisier, together with Laplace, made enquiries into the causes of animal heat; and carrying the principle of oxidation into their researches, attributed animal heat to the union of oxygen with hydrogen and carbon in the lungs during respiration. Lavoisier thus sums up his conclusions: The animal machine has three regulators—*respiration*, by which hydrogen and carbon are consumed and heat is produced; *transpiration*, by which temperature is lowered and the body is cooled; *digestion*, by which the blood regains what it had lost. Whilst this continued to be the generally accepted doctrine of animal heat down to about the year 1840, neuro-pathological

theories were still applied to the explanation of fever heat, except in France. Even in 1812 Sir Benj. Brodie adduced experimental evidence (in a paper published in the *Philosophical Transactions*) against the theory of the production of heat by the respiratory processes, and concluded that the source of heat must be sought for in the nervous system. And this was nearly fifty years before the celebrated experiment of Claude Bernard—division of the cervical cord of the sympathetic. Meanwhile, chemists and physicists were making rapid strides towards that great generalisation of modern times—the law of the conservation of energy. The time when that law was finally announced marks the end of our retrospect. During the period now under consideration—*i.e.*, from the middle of the 18th century to about the year 1850—great changes occurred and great advances were made in the views as to the nature of heat. The development of the dynamical theory of heat preceded, and was the ground of, those researches on which are based our present views of fever-pathology.

In 1757, Black established the doctrine of Latent Heat, having previously discovered carbonic acid gas, or, as Black called it, "fixed air." The analogy between the fixed and the free states of carbonic acid gas—fixed, *e.g.*, in marble, free when the marble is acted upon by an acid, —and the sensible and latent states of heat—fixed or latent in steam, free or sensible in the water condensed from steam,—led to the conception of heat as a *substance;* a substance which took part in the constitution of matter. But, since gases were found to have weight, whereas the presence of heat in a body could not be detected by the balance, the substance, heat, was called "an imponderable substance." And electricity, light,

and even the so-called vital force, were included in the list of "imponderables."

Count Romford, at the end of the last century, by his well-known and beautiful experimental investigations, disproved the materialistic theory of heat. There is this in Romford's work and results which is of special interest to us: he saw clearly that the force of animals was derived from their food, and that no *creation* of force took place in the animal body. Davey followed Romford, and taking the materialists on their own ground, upset them completely. The sum of the results of these observers may be thus expressed: that when work is spent in overcoming friction, the amount of heat produced is proportioned to the work spent. Then came the expression of this relation numerically. The experiments and measurements of Dr. Joule, of Manchester, established in 1843 the mechanical equivalent of heat. At this time, a member of our own profession, a practising surgeon at Heilbronn, in Germany—Dr. J. R. Mayer—was busy formulating his doctrine of the unity and co-relation of all forces—whether mechanical, chemical, thermal, or electrical. He appears to have arrived at this notion of the interdependence of forces from his reflections on the relations between the thermal, chemical, and mechanical phenomena of the body and the force contained in food. Mayer worked out theoretically and enunciated the convertibility and indestructibility of force. Joule contributed the experimental basis upon which the doctrine rests as a law of science. The law was, at this time, called the Law of Conservation of Force.

Heat, up to this time, was matter; the view accepted now is the antithesis: Heat is a mode of motion—translated mechanical motion. The idea of heat as a *vibratory*

motion of the particles of a body had not yet taken deep root—although individual thinkers (notably Newton and Cavendish) had, for a long time before, conceived even this state of things in reference to heat.

Meanwhile, the progress of organic chemistry had been great. A connection between the constituents of food, the heat of the body, and the chemical processes going on in the *tissues* of the body was established. Accordingly, the facts of organic or animal chemistry, together with the doctrines of the Dynamical theory of Heat, were the bases of the researches of Parkes, Senator, Liebermeister, Virchow, Burdon Sanderson, and others; the foundation upon which was elaborated most of our present knowledge of the Pyrexial state.

It is now some ten years since any noteworthy addition was made to our knowledge of the nature of Fever. It is as if the oars rested after so desperate and brilliant a struggle over so long a course. The knowledge acquired by the men just named is our present knowledge. If, then, we recount our present knowledge of the pathology of the Pyrexial state, we shall at once complete our retrospect of the work of the past, and enter upon a review of the knowledge of the present. But, not only is the time too short for such an exposition; this knowledge must be familiar to us all. Wherefore, a brief summary must suffice; and our review shall be chiefly of the work of the present in chemistry and physics, as a prelude to the forecast which is to follow. Of the *work* of Parkes and his worthy contemporaries it will be enough to say, that the experiment was laborious and the observations were accurate, and both were perfectly adapted to secure the ends held in view; that the conclusions arrived at were obtained by just and scientific reasoning such

as Bacon and Boerhaave might have taken for their model. Of the knowledge which came out of it all, here is the epitome. Premising that the elevation of temperature is due to increased production of heat, it was established:

1. That the nitrogen discharge of health is in direct proportion to the nitrogen ingestion.
2. That a man in fever discharges more nitrogen than a man in health—both being on the same nitrogenous diet.
3. That the excess of nitrogen = $\frac{3}{4}$ of the normal discharge—*i.e.* the nitrogen excess in fever is more by $\frac{3}{4}$ of the normal discharge. (Unruh.)
4. That, in fever, there is an excess of discharge of carbonic acid which is not accounted for by increased respiration.
5. That the amount of potassium salts discharged in fever is three or four times greater than the amount discharged in non-febrile states. There is also an increased discharge of pigmentary matters in fever about equal in excess to the potassium salts. (Salkowski.)

How are these facts related?

In health, all the nitrogen discharge is derived from the food.

When there is practically no nitrogen income, as in fever, the nitrogen discharged (in excess) must come either from the circulating or the fixed albumen.

Now, in fever the nitrogen income is as defective as it is in inanition.

The circulating albumen is used up in fever. But, besides this, there is an additional disintegration of albumen which, from the fact of the increase in the discharge of potassium salts, probably takes place at the expense of blood corpuscles and muscle, or other tissue.

Wherefore, the tissue-origin of the increased production of heat in fever is now an accepted fact.

That there is an intimate relation between the states and activities of the nervous system as a whole, or of some of its parts, and the maintenance of fever heat there can also be no question. At present, however, this subject has not come within the range of experimental investigation.

We pass on, then, to inquire in what way physics and chemistry have carried on the work of the past, in this half-century, and what physics and chemistry are now doing towards a basis for the knowledge of the future.

And first we note a change in nomenclature, and a change in the forms of thought, for both sciences, from the time we last spoke of them—at the epoch of the enunciation of the law of Conservation of Force. Nor is this change confined to physics and chemistry: all other departments of knowledge that depend upon these sciences—and what departments do not?—must needs run with the stream. Physiology is now fully one with physics and chemistry in this respect.

The change is not a mere change of names for things. The change may be traced—through steps which need not now be detailed—to a wider and more exact conception, developed during the near past and the present, of the law of the Conservation of Force, and to wider

applications of the principle. The law as it now stands is called the law of the Conservation of *Energy*. As at present understood, its speciality, as it were, consists in the comprehensive and accurately scientific idea expressed by the word Energy. As we now define it, energy is the capacity for doing work. But energy is, after all, only a new name for a *quantity*, which came before us first during our retrospect of the mechanical work of the 17th century; an old conception, widened with the progress of science. It is essentially the old quantity by which the power of a moving body was expressed in the time of Galileo—the product of the mass into the square of the velocity; the *vis viva*, a term used by Liebnitz later on to express the same thing; the Kinetic energy of to-day. [$F = \frac{1}{2} M V^2$.] Heat is now regarded as one of the forms of energy; and the energy is believed to exist in a hot body in the form of rapid and irregular, though limited, motions of the invisible particles of the body—as Kinetic Energy. Energy may be measured in the form of heat; and every kind of energy can be converted into heat. All we know about heat is what occurs when it passes from one body to another, and we must be careful not to assume that when energy in the form of heat enters a substance, it exists in that substance as heat. The old notion of "latent heat" is exploded, and a new meaning has been given to that term. By the teaching of modern physics, all that we know about matter relates to the series of phenomena in which energy is transferred from one portion of matter to another; we know matter only as that which may have energy communicated to it from other matter, and which may, in its turn, communicate energy to other matter. All that we know about energy is relative; we can ascer-

tain the increase or diminution of energy, as a quantity, as any material system passes from one definite condition to another. There is no such thing as absolute energy; and even if there were such a thing, knowledge of the fact would be of no use to us, since all phenomena depend on the variations of the energy. These newer ideas and ways of physics have found their way into the thoughts and work of physiologists for more than these ten years past. And much has come out of it. For new knowledge in regard to the development and disposal of heat in the body has been gained, and still the newer kind of work is going on; so that in a little time much new knowledge may be available for use to the progress of Medicine.

Within recent years, a most important deduction from the law of Conservation of Energy has been made—a deductive principle we owe to Sir W. Thomson, and known as the law of the Dissipation of Energy. This law affirms (what also belongs to the Conservation of Energy) that the quantity of energy in the universe is invariable, and that energy can neither be created nor destroyed. But it goes on to affirm that every change in the form or distribution of energy is attended, on the whole, with a running down of energy—that is, a diminution in the amount of work it can be made to do. This is also expressed by the phrase "loss of potentiality." Thus, if mechanical energy be converted into heat, this heat cannot be entirely converted back again into work; and this is owing to the diffusive nature of heat. So that the result of continually converting mechanical motion into heat will be that the amount of work to be got out of the heat will be always growing less, until all the energy has taken the unavailable form of diffused heat.

That the further development and application of this law is pregnant with great consequences to physiology and pathology cannot admit of doubt. Already, by the help of this law, the chemist of to-day is busy seeking new explanations of the phenomenon of chemical combination. In the present, not only is chemistry gaining a great store of new facts as to the constitution and changes of organic bodies; the work in thermo-chemistry now going on must produce far-reaching results. And it is, in my opinion, here that we must look for help in the further advancement of fever-pathology.

It was, for a long time, the prerogative of chemists to be concerned with the smallest and ultimate particles of matter. Physics stepped in and usurped authority in this domain. And physics, ever since, has maintained the lead. But now chemists, bowing to the inevitable, are following the lead of the physicists, and are applying mechanical notions to the solution of chemical problems. The principle of the Dissipation of Energy is being applied to explain the reason why elements arrange themselves in certain groups to form chemical compounds. Vibration in unison, harmony of motion of atoms, consequent on the tendency in nature towards an equal distribution of energy, may, it is seriously suggested, be the cause of equilibrium amongst atoms and the forming of chemical compounds—without need of invoking a special kind of attraction, called "chemical affinity." The principle of the equivalence between mechanical work and heat has been extended so as to embrace all molecular actions; as, for instance, the motion of molecules during change of state, and during chemical changes. The principle of the mechanical equivalent of heat being supposed to be true for molecules as

well as for sensible masses, its application to molecular work has led to consequences which experiment confirms in a manner remarkably constant. The agreement between the theoretical deductions and the experimental verifications permits the application to all chemical phenomena, with something very like certainty, of that relationship which exists—according to physics—between heat lost and work done. And thus the celebrated French chemist, Berthelot, deduces from the law of Conservation of Energy his principle—the principle of the heat equivalence of chemical transformations.

It is almost startling when one realises what this new development of mechanico-chemistry means. Thus, in respect of chemical combination, molecules are precipitated upon one another with great velocity, and a disengagement of heat results. Each of the invisible molecular masses is the seat of different kinds of motion —motion of translation, of rotation, of vibration,—all motions which are ordinarily destroyed or transformed in the formation of a new compound. The distances between the molecules are changed, and consequently their reciprocal actions; new relations and conditions are evolved. The work done during all these various changes is expressed, just as in the case of sensible masses, by the heat disengaged. The mind needs to become accustomed before it can realise that things so subtle and delicate are now, in this present, the subjects of calculation in terms of *vis viva* and of experimental verification; still more, before it can realise the enormous influence which such researches must exert on the progress of science, and through this on the progress of Medicine.

Berthelot carried his investigations into the domain of

physiology, and established new facts in regard to the production of animal heat—or, at least, has given to certain facts a new importance. Here, we can only indicate and suggest, and that briefly, how these things may bear upon the knowledge of the febrile state of the future.

Here is the heat. What of the work out of which it comes?

Berthelot's first theorem, formulated as a basis for the calculation of animal heat, runs thus: "The heat developed by a living body, during any period of its existence, accomplished without the aid of any outside energy except what is derived from its food (oxygen and water included), is equal to the heat produced by the chemical metamorphoses of the immediate principles of its tissues and its food, diminished by the heat absorbed in the external work done by the living body."

The development (as we term it) of heat in the body is not an origination or production *de novo;* it is an alteration in the distribution and dissipation of energy. And this redistribution of energy is brought about chiefly by changes of a chemical nature. Now, these chemical changes are of very diverse kinds—oxidations, hydratations, de-hydratations, decompositions. Each of these reactions disengages or absorbs heat, and each in its own way and on its own account.

Hitherto, in the investigation of fever, attention has been directed almost exclusively to oxidations. But we must not pin ourselves down to the idea that, when we have established metamorphosis of particular tissues, and by oxidation, as a source of the heat in fever, we have set a limit to the source of fever-heat. Berthelot and others have found that, judged from the new point of

view—the calculation of the *vis viva* of molecular energies,—the influence of the other changes above-mentioned in the production of heat must be very great. For instance, decomposition is a source of heat quite independent of all oxidation. Hydrates of carbon, sugars and their analogues, disengage heat by decomposition alone, independently of oxidation. Fatty bodies can give rise to heat in the same manner, and also by simple hydratation. The albuminoid principles are amides, and, like amides, give rise to acute heat phenomena at the time of their hydratation with decomposition, or their de-hydratation with combination. And these phenomena occur without the absorption of any oxygen or the production of any carbonic acid.

These points have been merely glanced at. But the facts referred to, and others like them, will have to be taken into account in further researches as to the source of fever-heat.

One word must be said here as to the participation of the nervous system in the production of febrile heat. Hitherto nervous agency has been chiefly invoked in explanation of heat regulation; though recently an interaction between the nervous system and blood and tissues has been inferred in the production of heat in fever. *Tension in nerve centres* is a favorite expression of the day. But what *is* nerve-tension? Does anyone apprehend it? Can it be expressed as a state of things in which molecular motion and energy are concerned? If so, it may have some connection with fever-heat. It might be asked if nerve-tension is a normal or a diseased state; and if it is a diseased state, whether it is a cause or an effect of the febrile heat.

Our knowledge of the Pyrexial state is no doubt great;

and true, so far as it goes. Yet are we only on the threshold of the truth. For, as always happens in science, the problem gets more and more complicated in proportion as we penetrate into the actual conditions of natural phenomena.

It was discovered by Wanklyn, some years ago, that sodium heated in dry chlorine gas was not acted upon by the chlorine; no chloride of sodium could be found, and the sodium remained perfectly bright and metallic. Harold Dickson (of Oxford) has recently shown that carbonic oxide and oxygen, when mixed in the proper proportions, will *not* combine to form carbonic acid, even by means of an electric spark, *if* the gases have been thoroughly dried. No doubt, we have, most of us, performed the simple experiment of placing sodium in an atmosphere of chlorine, and have seen the sodium burn even without being heated; and therefore we have believed the fact. It *was* thought that we *knew* how sodium is acted upon by chlorine. *Now* we find we do not know, because, under certain conditions—the almost total absence of moisture,—chlorine will not act on sodium at all. And we don't know *why*.

It *was* a fact in astronomy that the earth and the planets move in ellipses round the sun. This is, however, only an approximately correct statement. The more the actual natural phenomena of astronomy have been penetrated, the more it has been found that the truth about the motions of the earth and planets is inconsistent with the original *simple* explanation. And so it is in all Nature.

"Knowledge grows, and thro' the ages one increasing purpose runs,
And the thoughts of men are widen'd with the process of the suns."

NOTES ON SOME MORBID APPEARANCES IN THE BRAIN AND SPINAL CORD OF A LIONESS.

BY

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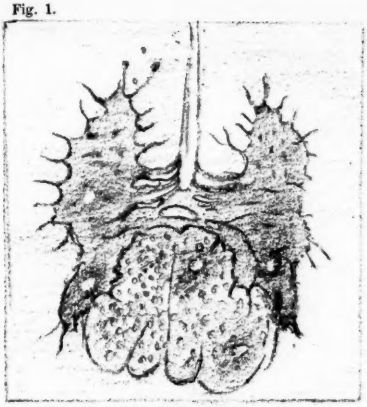
A FEW months ago I received from Adelaide, South Australia, portions of the brain and spinal cord of a lioness that had died with paralysis, at the Zoological Gardens of that city. The portions of the brain were taken from the posterior convolutions of the hemispheres, including the descending limb of the corpus callosum. Of the spinal cord, there was only a length of about three inches, including the medulla oblongata. The general characters of these portions of the nervous centres did not differ in appearance from those of the normal cord and brain in man, horse, sheep, calf, cat, and monkey, beyond what would arise out of the relative difference in size of the animals. There were, however, a few microscopical differences in the cord of the lioness which may here be described. The most obvious of these were observed in the size of the posterior columns, particularly of the wedge-shaped columns of Goll, which parts were not only larger, but the medullary fibres of which they were composed were unusually large, and their sheaths and axis cylinders more distinct than usual. (Fig. 1.) Besides these features, there were visible therein numerous points of miliary degeneration. The central commissure of the cord was broad, and exhibited strong bands of nerve fibres

Fig. 1.



LARGE SIZE OF POSTERIOR COLUMNS, AND OF THE TRANSVERSE COMMISSURE OF THE SPINAL CORD.

Fig. 2.

DILATED VESSELS, TUNNELS, AND LOSS OF SUBSTANCE IN THE CORPUS CALLOSUM.

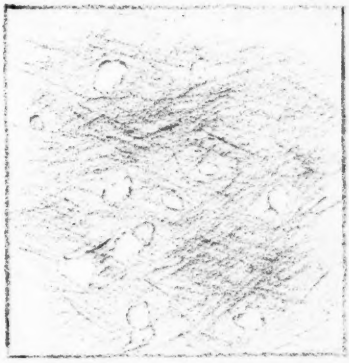
Fig. 3.

DILATED VESSELS AND GROUP OF MILIARY SCLEROSIS.

Fig. 5.

NERVE CELLS IN A STATE OF GRANULAR DEGENERATION.

Fig. 4.



SINGLE SPOTS OF MILIARY SCLEROSIS.

passing off into the cornua. A similar arrangement may be seen in the spinal cord of the horse. (Fig. 1.) The anterior half of the central commissure was notably wider than the posterior segment, whilst the central canal was larger than in the human cord; its lumen circular and spacious. The multipolar cells in the cornua did not differ materially in size or form from those of the human cord. The aspect of their nuclei and processes presented no diversity as to number or size, except where they had undergone granular degeneration. (Fig. 5.)

The measurements of the cells in the anterior horn were as follow: Width, from $\frac{1}{200}$th to $\frac{1}{2100}$th inch; length, $\frac{1}{218}$th to $\frac{1}{300}$th inch.

The vessels, more especially in the corpus callosum, and convolutions, gave evidence of repeated distension during life, with *post-mortem* contraction, leaving what have been termed "perivascular spaces."

Tunnels and holes were also observable in the cerebral substance, from whence vessels had disappeared. (Figs. 2 and 3.) In some parts extensive loss of substance was visible.

To these indications of chronic disease were to be added groups of spots of miliary degeneration in the medullary substance of the brain (Fig. 3), as well as in the grey substance of the cord, together with fragments of myelin—the myelin sheath of the nerve fibres being very distinct, and the axis cylinders strongly marked. (Fig. 1.)

The appearances which are here referred to, (Fig. 4) as miliary "sclerosis," or degeneration, have been held by distinguished pathologists (prominently so by Dr. Savage, of Bethlem Royal Hospital) to be purely *post-mortem* changes, attributable to alcohol, or other harden-

ing agents. I am not convinced of the correctness of this opinion, but it is only right to state that the brain and cord now under consideration had been preserved in spirits of wine. I may add, however, that a portion of cord of a calf, that had been in spirit about the same length of time, presented no trace of this lesion.

With much deference to the opinions of the eminent pathologists above alluded to, I would submit that, even if owing in any degree to the action of hardening agents, there must be a pre-existing morbid condition to account for its presence in some cases and its absence from others, under the employment of similar hardening agents. Further, I may say that bodies, in all respects resembling miliary sclerosis, if present, may be detected by staining media, in perfectly fresh substance of the brain and spinal cord.*

* See Dr. Batty Tuke, "On a handy Method of Examining Morbid Nervous Tissues Microscopically," *British Medical Journal*, Sept. 5th, 1874.

THE RELATIONS EXISTING BETWEEN FACIAL NEURALGIA, FRONTAL HEADACHE AND DENTAL CARIES.

BY

H. BOYLE RUNNALLS, M.R.C.S.,

Saltash.

DURING a comparatively short medical career, I have been astonished at the large number of cases which have come under my care owing their origin either to undeveloped teeth or to teeth undergoing a degenerative process, and that, on referring to my case-book, by finding that the percentage of such cases is on the increase. Some ten years ago I made a collection of human bones and teeth, which I obtained by excavating in the sand-hills on the west side of Crantock, a small village on the north coast of Cornwall. There is no record existing that will account for the presence of such a large quantity of bones there of both old and young human beings. The skeletons are found lying in all directions, and not separated from each other by anything but sand, and I could not find any relic to assist me in ascertaining when and why they were placed there. I obtained several hundred of the teeth, most of which were scattered loose; but some I found *in situ*, which fell out as soon as the surrounding sand was removed. All the lower maxillary bones which I was fortunate enough to find intact differed greatly from those now seen, inasmuch as they are very much larger and the fore part squarer. Those that I saw *in situ* contained the teeth in perfect position and condition, with no jambing of tooth upon tooth.

The upper edges of the incisors were regular, and presented no notches or dents. The other bones were correspondingly large, particularly the scapulæ, humeri, and femora. I could not collect sufficient of the latter in a perfect state to take an average length. None of the teeth that I found presented even the slightest appearance of deterioration, although I searched most assiduously to find even one that did. Although noticing the absence of decay in the teeth, and the larger size of the jaw bones, I only attributed it then to a freak of nature; but since I have become better acquainted with the human family, and noticed the great rarity of good teeth and the early appearance of decay, I have begun to think there must be cause and effect to account for the vast difference. Is not this one of the proofs of the gradual change which is taking place in the construction of the human frame? Even in the present day we find those members of the human family who use their teeth for chewing coarse uncooked foods possess masticatory muscles which are prominent and largely developed, and the bones to which they are attached developed in a like manner, and teeth which rarely present any appearance of decay. In the civilised races, who feed upon soft cooked foods, we find that the jaws become smaller and more contracted, with a consequent pressing of tooth upon tooth, and decay appearing at a very early age. The decay increases in the younger members as we ascend in the social scale; but it is difficult to arrive at the actual percentage, for the dentist places an obstacle at so doing by removing some of the teeth of the children of the wealthier classes, which is recognised as a necessity if it is wished to preserve the remainder.

This alteration in the size of the mouth and condition

of the teeth, gradual as it is, is accountable for many abnormal gastric conditions, but more particularly for those pains in the regions of the face and cranium, which, I think, are wrongly called "neuralgia," when that term is used to designate a distinct disease, and to be treated as such. Personally, I have never seen a case of "neuralgia" in which I did not find that it was produced by either a carious fang, periostitis of tooth cavity, carious tooth, dead tooth, undeveloped wisdom-tooth, exostosis of fang producing pressure on nerve, hardening of dentine, eventually producing closure of pulp cavity and pressure on the nerve, or too close approximation of the teeth, and which disease always disappeared when these causes were removed. (Of course I do not mean those cases which may arise from pressure of some growth upon a nerve, or where a nerve may be implicated with carious bone or a scar.) I daresay the following clinical records will bring to the readers' recollection many like cases; and my only excuse for so detailing them is to strongly impress the necessity for operative treatment, and not the constant administration of "bromide" and its allies, which often aggravate the dyspeptic symptoms from which the patient suffers:

B. H., female, age 30. When I first saw her she complained of pain in region of the ovaries, and constant pain in forehead; said she was very subject to "hysteria." As she told me she had an offensive vaginal discharge, I examined the canal, but found nothing but what ordinary cleanliness would remove. On examining her mouth I found the teeth were artificial, and had been fitted on the stumps of carious teeth. I advised removal of all the stumps, which she allowed me to do, and from that time all the aches and pains disappeared, and the hysteria also.

She had had a varied experience of medical skill for seven years, and must have taken in that time gallons of physic.

B. S., female, age 28. Complained of earache when I first saw her; otherwise appeared to be perfectly healthy. Could not discover any cause in the external meatus to account for the pain. The throat was in a normal condition, and the teeth fairly good. Not feeling satisfied as to the cause of the pain, I gave her a mixture of olive oil and opium to drop into the ear, but which produced no benefit. I continued this treatment for some time, when one day she complained of pain over the angle of the jaw on the same side that the ear ached. I carefully examined the teeth and face, and again was baffled as to the cause of it; but a day or so after I found that the tissues behind the last visible molar were swollen. I incised them, which resulted in the discharge of a small quantity of pus. I naturally thought there must be diseased bone somewhere near, and the probe proved that I was right, but not the kind of bone I thought it was. On making freer incisions I found that my carious bone was a carious wisdom-tooth, which had not appeared above the gums. I extracted it, and the earache almost instantly disappeared.

G. T., aged 32, female. When she came for my advice, complained of constant faceache, complicated at times with hysteria. Said she had suffered from it since the age of 19. Had resorted to numberless remedies, but never had any relief. The teeth, to my astonishment, were remarkably good, not one of them presenting any signs of decay. As the wisdom-teeth were not visible, and as she told me that they had never appeared, I made an incision over where they should be, and found the crowns about $\frac{1}{16}$th of an inch below the surface of the

gums. Thinking the removal of the tension might give relief, I waited for a fortnight, at the end of which time I extracted both, as the pains in face had not decreased. After their removal all the pains disappeared, and she has never had a return of them.

Hamilton Moore, Esq., practising as a dentist in Paris, informed me that a lady was sent to him by a renowned European oculist, for the purpose of having the stump of the left canine extracted. On its removal the lady immediately exclaimed, "Why, I can see again!" This lady had lost the sight in the left eye for several years, and had begun to lose all hope of its recovery.

I could relate numbers of like cases, and cases of dyspepsia, where ailments all disappeared when the teeth were attended to. It is an invariable rule with me now to carefully examine the teeth when a neuralgic or dyspeptic patient puts in an appearance, and I absolutely refuse to give physic when I find a tooth presents any appearance of caries or retarded growth. I frequently get cases of facial neuralgia where the teeth have been filled, and I always strongly advise their removal, with an invariable result of instant relief, as I have always found that the teeth have been ineffectually filled, either because the whole of the carious tissue had not been removed previous to the tooth being filled, or because there was periostitis of the fang existing with the carious cavity. I remember a case well exemplifying the foregoing. A lady came for my advice as to whether a carious tooth should be removed or filled. By the side of it was another which had been previously filled. From the appearance of the tooth that had been filled, and the condition of the gums around it, I felt convinced that it was the offender. I removed it, and found my surmise was right,

there being an extensive accumulation of pus at the bottom of the cavity, and it had welled up into the pulp cavity underneath the filling. The other tooth was filled, and the neuralgia made a speedy exit. No one would dream of giving physic for pain in the knee resulting from hip-joint disease, or pain at top of penis and in the groins resulting from stone in the bladder, and I am of an opinion that it is quite as hopeless to give physic with the intention of curing "facial neuralgia;" but notwithstanding, there are gallons upon gallons of nauseous drugs annually prescribed which may be alphabetically arranged from assafœtida to valerian, when the performance of a quick but painful operation would result in an almost instantaneous cure. Of course I would not extract a tooth, or advise its removal, if I thought the tooth might be saved by being properly filled; but unless the tooth is carefully and tenderly dealt with during the cleaning and filling process, it would be much preferable to have it extracted. There are many dentists, but few good ones—at any rate, that is my experience,—and rather than allow a tooth to be treated as a lump of ivory, forgetting altogether its delicate construction, I should always advise extraction, unless the person's means would allow them to place themselves under the care of a thoroughly scientific man.

Clinical Records.

HISTORY OF A CASE OF LYMPHADENOSIS.

By JOHN MEREDITH, M.D., Wellington, Somerset.

IN the beginning of the year 1877, W. H. consulted me about a swelling on his neck, size of a hen's egg. He was 21 years of age, and was engaged as an assistant in one of the large warehouses of the metropolis. He was frequently employed in the underground rooms of the establishment, in which the atmosphere was stuffy and close. He had been in the same employ during the previous year. He led, as was stated, "a rather fast life" during his stay in London; but he considered himself healthy and strong, until towards the end of 1876.

When he visited his home in the country, that Christmas, he was noticed to be a little anæmic; tongue, pale, furred, and slightly tremulous; quick, irritable pulse. He, however, returned to his duties after his few days' holiday; but he said that he was "not up to the mark." On the 9th of February, 1877, while having a bit of play with one of his fellow-assistants, he felt a sharp pain on the right side of his neck, owing to a sudden turn of his head during the tussle. Next morning a painful swelling was noticed behind the angle of the jaw. It remained for a few days, then seemed gradually to disappear, but did not really do so completely.

After this the right ear became painful, an abscess formed and burst, seemed to recover for a while, then

became painful, formed and burst again; and this happened several times during the spring of 1877. The swelling behind the angle of the jaw began to increase in size. The patient had to give up business in town and return to his home. This occurred about the end of April. He had been taking various forms of iron tonics before he came home, and continued doing so afterwards. The tumour was movable, and felt hard; no fluctuation; it was evidently glandular. From this time on, I had the care of the case. I took the temperature occasionally, and found it one to three degrees above normal; pulse rapid, from 100 to 120. Dr. Andrew Clark examined the case early in June, and said he feared the presence of a tubercular element, but that the lungs at that time were all right.

On the 12th of June it was noted that he was getting thinner, and had occasional nocturnal sweatings. The lungs seemed healthy. There was no perceptible indication of tubercle. The condition of the heart was normal. The patient had, except at times, a fair appetite, and his bowels acted regularly. There was no sign of disease about either the head or spine, nor in connection with the generative organs.

The family history was considered good; no history of scrofula or phthisis in it; and the young man had been well cared for and lived well.

The chief thing now was the tumour in the neck. It had been increasing in size among the deeper tissues, as well as outwardly. It was seen bulging in the pharynx, and compressing the nasal orifice. He became subject to fits of nausea and vomiting at times, and felt sick at once when the lower edge of the tumour was pressed.

A glandular swelling formed on the left side after a

while. The date upon which this was noticed first is not mentioned. On the 25th of June the following note was made: Patient lying in bed; pulse, 100; temperature, normal. Lungs and heart seem healthy. The tumour on the right side remains about the same in regard to size. It appeared to press less on the pharynx than it did. There is a great deal of muco-purulent discharge, and at times nausea and vomiting. The tumour on the left side, behind the ear, is increasing in size.

July 18th. The patient was by the seaside at Teignmouth, for a change. I saw him there on this date, before he left his bed. He complained of feeling very weak. He had been taking chloral for the relief of pain, and it had disagreed with him. He was often taking some anodyne or another. His pulse this day was 120; temperature, 99.5°; respiration, normal; tongue, moist. Complained much of a "thickening in the throat," with muco-purulent discharge adhering to the fauces; but he had not expectorated much of it lately. Suffered from paroxysms of pain in the neck and head: the pain seemed to spread from the glandular tumour in the neck, sometimes very severe at night.

The state of the chest was normal, save that respiration murmur appeared feeble on the right side, and the cardiac sounds louder; percussion sound normal in both lungs. There was no sinking in of the chest-walls.

The tumours on each side of the neck further increased in size, and were tender to the touch. I could detect no fluctuation at the time. He was able to take a fair quantity of food, notwithstanding the suffering.

Bromides with hyoscyamus were given, with the view of affording relief from this. The patient's strength was day by day decreasing.

July 22nd he returned home. On examining him that evening I found decided dulness on percussion over the left side of the chest, with decreased respiratory sound everywhere.

July 28th. The dulness had disappeared; respiratory murmur, feeble; pulse, 112 to 120; temperature, 99.5°. Four days later his condition seemed better. He had rallied, said he felt better, able to take a good amount of food, and did not require chlorodyne or anything of the kind to procure rest and sleep. Suffered from fits of gloomy despondency. The urine was examined this day, and found of normal character.

On the 10th of August, Mr. John Gay, of Finsbury Place, London, saw the patient in consultation (Mr. W. Edwards acting on my behalf, owing to my absence), and it was decided to incise both tumours, as each had now a fluctuating area.

The incisions gave vent to a quantity of serous fluid. It was expected it would have proved to be a sort of thin, curdy substance. The growth on the right side had an opening into the pharynx, which was distinctly felt behind the right tonsil. Through this orifice the glandular tumour had discharged portions of its contents.

After the incisions had emptied all that could escape, a considerable quantity of glandular growth remained, which had not melted down.

Note made on the 14th of the month is to the following effect: "There has been a great amount of discharge from the tumour on the right side, and there is a slough to come away; the discharge is mainly serous. The pulse last evening was 140; temperature, 103°. Still, the breathing seemed normal; in no way very hurried. There was nothing particular to observe in regard to the con-

dition of the lungs, except that vocal resonance appeared somewhat increased. This morning the pulse was 136, and temperature 100.3°. The patient faints upon the slightest exertion; says that he does not feel the pain in the throat and the swellings as much as he did before the latter were incised."

At this time he was labouring under the combined exhaustion of rapid pulse, abnormal temperature, and great discharge from the glandular enlargement of the neck. Against this had to be placed the fact that he was able to take a good amount of nourishment, including beef-tea and other articles. I scarcely expected he would be able to live many days, judging from his then condition.

On the 17th of August he seemed better; been improving for three days. Pulse, 120; temperature of the body, normal. Discharge from the right tumour more purulent in character than heretofore; sleeps better, and said he was able to swallow his food with comparative ease. Very little discharge from the tumour on the left side.

August 24th. A severe rigor set in on the afternoon of that day. Temperature rose to 104°, and the pulse to 160 or thereabouts; I was not able to count it correctly. The gland on the right side of the neck had a bag-like patch of fluctuation: the contents burst during the night, and discharged through the opening previously made. The edges of the sore through which the discharge escaped were red and prominent, and a piece was clipped off and microscopically examined, with the result of showing that it was only "proud flesh," with no sign of malignancy.

On the 27th of August he rallied, and appeared to have got over the exacerbation which set in three days

before. On the evening of the 24th there was a good deal of bilious vomiting, and bilious matter was passed in the stools. The two succeeding nights were restless, chiefly from rheumatic pains in the right shoulder; but, as stated, the patient seemed better on the 27th. He insisted upon those about him giving him alcohol in some form or another, and appeared to be taking more than was good for him.

The tumour on the right side of the neck seemed as if it was breaking down by suppuration; but there was little change in the character of the swelling on the left side. There was nothing to describe in the way of change until the middle of September, when the suppurative discharge from the right tumour gave place to a discharge of a serous character; and on the 23rd this stopped, and the orifice closed; but it was soon found out that it imprisoned some fluid, which readily fluctuated.

After all the discharge, there was no perceptible decrease of the size of the glandular swelling, beyond that now it did not cause much inconvenience through pressing on the pharynx. The state of the left tumour remained much the same, discharging some semi-serous fluid every day.

On the 25th of September the pulse was 130 and upwards; temperature, 99°. There was a cheerful brightness about the patient's face and the tone of his voice that day. The condition of the lungs remained about the same; vesicular breathing, feeble; appetite good; bowels and kidneys acting regularly.

His blood, when examined under the microscope, showed a great deficiency in the number of the red corpuscles. The patient was weak and thin. At this time he was taken to stay at a house on the Blackdown

Hills; but no particular improvement resulted from the move. At the end of October his condition was as follows: Pulse, 120 and sometimes higher; temperature, 99.5°; breathing, normal. There was a peculiar condition of the chest: no complaint of pain: woodeny sound on percussion: tubular respiration: hardly any vesicular breathing anywhere.

Under Mr. Gay's instructions, the glandular swellings were injected with a dilute solution of hyd. pernit., and ten-minim doses of tinct. mur. fer. given internally thrice daily; but this was followed by an attack of headache every time it was tried. The injection of the solution induced some suppuration in the left tumour; but none would take place in the one on the right, even when tried more than once. No marked change otherwise.

On the 29th of October the percussion-sound seemed clearer over the chest: there was a slight cough and some expectoration; but no râles could be detected, and there were none at the date of the next entry, one week later. The patient was then able to take food fairly well, for him; was always sleepy in the daytime, occasionally wakeful in the evenings; getting thinner and thinner; clubbing of the finger-nails very clear. The discharge from the tumours thin and serous again. Occasionally he experienced twinges of pain spreading from the right tumour down the arm and side.

On the 22nd of November the pulse was 130 and over; temperature, 100°. For a few days before this the pulse was as low as 112, with normal temperature, but it was only for a short time. The tumour appeared to soften. Had a cold, with some cough and expectoration; still, I was not able to detect râles in the chest. When asleep, he had an irregular stridulous breathing; was

weak, and slept much. I painted the tumours with tincture of iodine twice a week, but with no advantage to the patient. He took a fair quantity of cod-liver oil for a time; then it was exchanged for koumiss, which in three days had to be discontinued, as it caused biliousness. All medicines were stopped for a time; then resumed in the form of cod oil and fer. redact. The patient, however, often rebelled against anything of the kind, and he was not pressed to take any when not disposed to do so. The "spasms," as he described the attacks of pain, extending from the left cervical tumour down the shoulder and arm, harassed him frequently: sometimes they lasted an hour or more; at other times, but a few minutes.

There was little to notice from day to day. The glandular swellings on each side of the neck bulged out to the level of the shoulders.

On the 24th of December it was noted that the general emaciation of the body was most marked. Appetite capricious: some days very little food was taken, and the patient was gloomy and despondent; at others, tolerably bright and hopeful. The neuralgic spasms connected with the left side became more frequent, and lasted for hours. The swelling had an angry appearance. The "spasms" recurred if food was not supplied punctually. At times the patient swooned away in those attacks of pain, and remained in a semi-conscious state for a long time; and was occasionally carried from his chair upstairs to his bed in that condition. His pulse at this period ranged from 116 to 140; the temperature almost always a little above normal.

An abscess formed over the left clavicle. The pressure of the left tumour on the pharynx increased, and gave rise to hawking expectoration.

The tumour on the right side seemed quiet. By the 11th of January, 1878, the gland above the left clavicle was much enlarged—formed a glandular cyst—skin over it red and thin. As it caused discomfort, I tapped it by trocar and canula, and drew off five fluid ounces of clear straw-coloured fluid; no pus or blood. The fluid was of neutral reaction, highly albuminous, and contained a great deal of chlorides.

Other glands of the neck had swollen before this one, and their contents had been withdrawn by incisions, as was done in the first instance by Mr. Gay, and in every case the character of the fluid was similar to the above. In regard to one sample, Mr. Gay wrote that "the fluid had an alkaline reaction, and contained a large number of blood globules, some of which were exceedingly pale, or rather white, and others moderately red. There was an intermixture of others of every intervening shade of colour. There were few lymph corpuscles, and a considerable quantity of albumen. A blood-clot separated from the fluid, which was very tough, and consisted of white and red blood globules and some few lymph cells. It was toughened by the albumen."

At this time there was very little discharge from the glandular enlargement on the right side; what was, seemed to have been clear glandular secretions, similar to what was poured out by the one on the left.

Lymph-secreting, apparently, was the work of the system, which, owing to the waste of it, was producing rapid emaciation of the body. The blood of the young man was being diverted from its flesh-forming function by the inordinate activity of the hypertrophied cervical glands, and thown away in the form of serum.

What could have been the exciting and sustaining cause of this?

January 17th. The urine had been examined at intervals since the commencement of the illness, but it was always found in normal condition. On this date its sp. gr. was 1015, with some earthy phosphates; but there was nothing special to note in this respect.

The patient was not entirely confined to bed until a couple of days ago. He slept a great deal, in short snatches; the expectoration from the throat was excessive, and prevented long periods of rest. The spleen and liver seemed of ordinary size, no pain or tenderness about them. There was no enlargement of the glands anywhere, except in the cervical regions.

February 10th. The patient remarked that morning that it was just that day twelve months he had that "crick" in the neck, which began all his troubles. As to progress, there was little to note. The discharge from the right side stopped some time previous to the above date, and had not returned. The glandular enlargement on this side spread forward under the chin: it was not painful, except when handled.

There were two glandular openings on the left side, discharging serous fluid. Glands in the axillæ were noticed to be enlarged. On the 6th, pain was complained of at the base of the left lung, acute when coughing; and there was a space indicating marked dulness on percussion, and moist râles when examined by stethoscope, with marked difficulty of breathing. The patient's body was extremely emaciated, and complete faintness followed upon any exertion, even trying to change position in bed. He became rapidly worse; the pneumonic area of the left lung increased, until by the 12th there was only a small margin in front which gave anything like normal resonance. No complaint of pain; complained much of

the pressure of the tumours on the gullet preventing deglutition; hardly a drop of fluid could pass. Death resulted on the evening of the 13th. The intelligence continued unaltered, to all appearance, until within a couple of hours of the heart's action stopping.

Two or three days before death, I noticed that the margin of the glandular sores in the neck were no longer red: they had become pale; the red blood, what there was of it left, being, no doubt, diverted into the left pulmonary space, blocking it.

Examining the body after death, I found no appreciable enlargement of the spleen or liver; no glandular enlargement anywhere, except in the axillary and cervical regions: these last were hard, and dipping graspingly into the deeper parts of the throat. No further examination was permitted. Had there been such an opportunity, I have little doubt I should have found that the cervical tumours compressed and interfered with the course of the pneumogastric nerve, accounting for the pains complained of; and discovered growths disseminated in the substance of the lungs, giving rise, probably, to the peculiar resonance and respiratory murmur mentioned, and inducing in the left lung the pneumonic condition which ended fatally.

As for treatment, everything that could fairly be suggested was tried, but nothing proved of any particular use. One distinguished medical practitioner to whom I spoke about the case, at the Manchester Meeting of the British Medical Association, said: "No matter what is done in these cases, they always end fatally; but try calcüchlor." I did so, but it did no good.

I have not had so marked a case of Lymphadenosis as this to attend to either before or since; and the

literature of the disorder, although considerable, helps very little towards elucidating its natural history. Looking at the case with the assistance of information which has come to light since, I am disposed to class Lymphadenosis among the filth diseases, and consequently preventible.

In Dr. Gower's article on this disease (Hodgkins), in *Reynolds's System of Medicine*, the enumeration of the "earliest symptoms" points irresistibly, according to my thinking, to the chief cause being persistent exposure to some marked insanitary condition or other. That organs take on excessive action and increase in size, to compensate for shortcomings on the part of other organs, is a recognised fact in medicine; but that they take on excess of action and persist in it, not because of other organs' defect, but owing to some vitiating, noxious influences, often not comprehended, is, perhaps, not equally well recognised. This form of action seems like a desperate strain to overcome a difficulty, so as to secure again the equable level.

Many of us, when exposed to unpleasant odours, especially such things as sewer gas, experience excited action of the salivary glands, and saliva flows freely. The sore throats frequently resulting from such exposure are attended by thickening of the cervical glands; and enlargement of the tonsils is common in such cases, likewise of the salivary glands.

The rush of saliva which sanitary inspectors experience in visiting ill-smelling localities is but the desperate strain of excretory organs to repel an uncongenial intruder; and he whose sense of taste and smell is not quick on such an emergency is to be pitied, and there are many such. The intrusion may result in possession being taken in

some form or another of blood poisoning; a condition being then established when "a crick," or some apparently minor action, is sufficient to determine the form of a fatal disorganising force.

Trousseau's view is that the disease at first is local, started by local irritation; and Dr. Gower's, that it may be local and constitutional in origin; and the particulars they adduce amount to nothing decidedly clear and specific.

Although I am not able to give an account of the condition of the various organs of my patient's body by a regular *post-mortem* examination, still, I can describe the hygienic conditions under which he lived.

At the commencement of the paper I mentioned that W. H. was said to have led a rather "fast life" when in London; but there was no history of gonorrhœa, chancre, or syphilis, and there was no reason to suspect his ever having had anything of the kind. The "fast life," whatever it was, could have been nothing vitally injurious. Still, opinions may differ on this. But, like others, he had to live for hours each day in ill-ventilated places when in the discharge of his work.

Next to this must be considered the sanitary influences under which he lived at his father's house in the country. This house, although to all outward appearance it seemed everything that could be desired as a family dwelling—roomy, well-furnished, and cleanly,—yet it possessed serious sanitary defects: 1. The water used was bad, not fit for use, yet was used. It was derived from a well, around which was a network of brick drains, in constant use. 2. Sewer air entered the house through the overflow of the rain-water cisterns under the kitchen floor. 3. Through the lead pipe of an upstairs water-

closet, of the old **D**-trap and iron-container sort: the pipe was eaten through, and had many small holes. 4. The pantry sink-pipe smelt badly when the plug was out: it passed, like the closet one, into the main drain, without any syphon or other form of check. 5. A small wash-hand place in the hall was connected with the drains in the same way, and smelt badly at times unless the plug was fixed in. 6. Under the back stairs of the house, and close to a couple of rooms in which the family lived most of their time, a bad smell was often noticed, and no one was able to say where it came from. However, after some years the house changed owners; and in September, 1884, assisted by the present possessor, I was able to make a thorough examination of the house, when the state of things just enumerated was brought to light; and we further found that the smell under the stairs was caused by sewer gas passing along a rat-hole from a drain, through the wall near the foundation, up under the floor into the place, taking its share with the rest in poisoning the air of the house.

The defects, I have no doubt, existed in 1877 8, and had done so for years before, although their deleterious influence was not suspected.

Cases of illness were always slow in recovering in the house, and its occupants frequently appeared languid, and often ailing. Thus, poor W. H. had the odds against him in every way. It might be asked why improvement did not appear when the patient was away on the Blackdown Hills or at the seaside. To this I would reply that both the glands and the system generally had been constrained into a bad form and habit—through long immersion in an impure atmosphere—from which they could not break off.

A CASE OF ADDISON'S DISEASE.

By JAMES E. PRICHARD, B.A., M.B. Oxon., Bristol.

The patient was a man fifty-nine years of age, whose health began to fail twelve years ago; since which time he suffered from occasional attacks of great weakness, accompanied by certain symptoms of faulty digestion; viz., complete loss of appetite, very furred tongue, vomiting, constipation, and sometimes slight jaundice. His wife also told me that she had noticed a brown rash about his forearms, which disappeared as he got better.

I saw him first on the 6th of January, 1885. His present attack began, he told me, just before Christmas. He was confined to bed, and excessively weak, so that moving about, or even talking, seemed almost too great an effort for him. Also he was very emaciated, and had, he thought, been losing flesh rapidly during the last month. His skin was dry and wrinkled, and of a peculiar pallor. Along the knuckles of both hands there was a little irregular brown mottling, but no bronzing elsewhere that I then detected. The conjunctivæ were slightly jaundiced.

Physical examination of his heart and lungs revealed nothing. The pulse was slow and compressible, between 60 and 70, but quite regular.

With regard to the digestive organs, he complained of complete loss of appetite and frequent vomiting. His tongue was furred and his bowels confined. Owing to his being so thin, it was easy to feel the abdominal viscera. The liver seemed to be lower down than usual, but its area of dulness was rather diminished than increased. Firm pressure over this region gave him slight

pain, but he complained of no pain or tenderness elsewhere. I could make out no enlargement of the spleen. There was no albumen or sugar in the urine.

I saw him frequently during the course of his illness, but noted very little change in his symptoms. Sometimes he felt a little stronger, and during March came downstairs, and even walked out as far as his garden-gate. The jaundice disappeared, and the vomiting became less troublesome, but never quite ceased, and his appetite improved. However, at the end of March he had a relapse, and became rapidly so weak that he was unable to get out of bed, or even to sit up without assistance. Just before death he became a little incoherent in his speech, and died quietly on the 17th of April, with no further development of his symptoms.

Dr. Shingleton Smith saw this patient with me shortly before his death, and to him is due the credit of pointing out the nature of the case; for until then I thought I had to deal with a case of malignant disease in some way involving and displacing the liver.

On the 18th of April I made a *post-mortem* examination. I then noticed that, besides the bronzing on the knuckles, there were a few small irregular patches on the inner aspect of the thighs and on either side of the penis. These marks were very symmetrical in shape and position. The bladder was sacculated in a peculiar way. The heart was very small and pale, and indeed all the organs were pale, and almost entirely devoid of fat. I did not see any pigmentation upon any of the serous membranes, or on other parts of the body, beyond those I have mentioned. All the other organs, with the exception of the right supra-renal capsule and the vessels forming the cœliac axis (which I exhibited to

the Society), were apparently healthy. The right suprarenal capsule is little more than a small empty sac. Its investing membrane is slightly thickened, and was so firmly adherent at the top to the surrounding structures that it was torn in removing it, and the contents which were fluid escaped. The left capsule does not seem to be affected. I have had one or two opportunities since of examining these organs in other bodies, and I think the left capsule is slightly more pigmented and a little smaller than usual. The vessels forming the cœliac axis are curiously dilated into pouches, and their coats separated by calcareous plates.

Under the microscope there appears a little increase in the fibrous tissue, and here and there a few fat cells towards the central portion of the gland. All that can be said is that the capsule is atrophied.

This, then, is a case of uncomplicated Addison's disease; and its symptoms, viz., the great prostration, the vomiting, the general pallor of skin, and the pigmentation, were, I think, typical, and, with the exception of the last, well marked.

There are, however, one or two points to which I wish to refer. Firstly, my patient was of more advanced age than the subjects of this disease generally are. In the cases recorded by Addison himself, three only were above the age of fifty, and in two of the three one capsule alone was affected. The ages of the rest were below forty. In the present instance, also, the right capsule alone is very seriously affected. This fact may, perhaps, also explain the very chronic course which the case ran, and also its recurring exacerbations and remissions.

Then as to the anæmia. This was not a very prominent feature in the case. My patient never complained

of any of those symptoms which we usually associate with anæmia. There was no breathlessness upon exertion, no palpitation, nor any abnormal condition of spleen or thyroid; so that I am inclined to think that the colour of the skin is due to a general pigmentary change rather than to any deficiency in blood corpuscles, or any alteration in the relative numbers of white and red. There was, at any rate, no increase in the proportion of white corpuscles in this case. The conjunctivæ are said to have a "pearly" appearance, due, I believe, to contrast with the more deeply pigmented skin, and not to any intrinsic whiteness of the conjunctivæ themselves, just as the whites of negroes' eyes are much more noticeable than those of white men.

There is one point in the pathology of Addison's disease to which I wish to draw your attention, and that is the nature of the lesion in the capsules themselves. It is maintained by some that this is specific, and certain of our text-books even define Addison's disease as "tubercular disease of the supra-renal capsules." In looking through Addison's own cases, I was struck, not at all by the uniformity, but by the great variety of the causes which gave rise to the symptoms. In several it was cancer, in one inflammation, in one tubercle, and in another an effusion of blood, or "kind of apoplexy," as he describes it, of the gland. In this case there is certainly no evidence that the disease is tubercular. It corresponds to the description given by Dr. Wilks at a meeting of the Pathological Society in May last, at which this subject was discussed; viz., "a disease analogous to cirrhosis of the kidney or liver, accompanied by a cell growth of inflammatory type." At that meeting Dr. Bristowe, the President (from an earlier edition of whose

work I have quoted the definition of the disease), said: "It appeared that the theory that the disease is tubercular is incorrect, and that the symptoms might be produced by any destructive lesion of the capsules;" and that, I think, must have been the conclusion at which Addison arrived, and to which the balance of evidence seems to incline.

In bringing this subject of Addison's disease before the Society, I purposely avoided any allusion to the Sympathetic; and for this reason, that though no doubt the part which the Sympathetic has to play in the production of the outward and visible signs of the disease is most interesting and important, yet it is still far from clear that there is always a structural change in the semi-lunar ganglia or the nerve-tissues in the neighbourhood, whereas it is scarcely open to doubt that some sort of morbid process occurs in the capsules themselves. I preferred wading in the shallows, where I could make out something of the objects at the bottom, to floundering about out of my depth in the sea of Sympathetic pathology.

Notes on Books.

The British Pharmacopœia. London: SPOTTISWOODE and Co. 1885.

The General Medical Council resembles nothing so much as a female elephant confined in a menagerie. Its size is great, and its separate members individually imposing; its maintenance is highly expensive, and its rapacity large; its feeble acts ludicrously disproportionate to its capability, and its fecundity extraordinarily low! At length, after some years of gestation and a lingering labour, the Medical Council has been delivered of an offspring, of which, to tell the truth, if it has no reason to be ashamed, neither has it cause to be unduly proud.

The work will have been studied more or less closely by all practitioners ere this, and a detailed notice of it is, therefore, unnecessary; but we may be permitted to make a few remarks on some of the more striking features. Judged as a whole, the new edition is a marked advance upon its predecessor in respect of the more consistent and more scientific chemical nomenclature now employed, of improved pharmaceutical processes and preparations, of some omissions, and, most of all, in respect of the many additions.

The change in chemical nomenclature does not call for special remark from the medical point of view, but to the student it is a convenience to find substances called by the same names by his lecturers on Chemistry and Materia Medica respectively. The improvements in pharmaceutical processes and preparations are numerous, especially in preparations, and consist principally in the "standardising" of such galenical preparations as the extract and tincture of nux vomica, the liquid extract of cinchona, and cherry-laurel water: these now contain a definite percentage of their active ingredients, which will lead to their employment with increased certainty and satisfaction. Most of the liquors have been altered from the strength of 4 grains to the ounce to a

solution of the strength of 1 per cent.: this is of no advantage whatever, and increasing the strength of solutions may lead to the occurrence of accidents; the solution of morphia for hypodermic purposes is in the same way increased in strength from 1 grain in 12 minims to 1 grain in 10 minims, and here the danger from overdose is still more likely. At all times a solution arranged upon a duodecimal base is more conveniently divisible than one upon a decimal base: had mankind, like certain giants, been furnished with six instead of five fingers, our numerical system would have been more generally convenient.

The formulæ for pulv. glycyrrhizæ co. and pulv. elaterini co. are improvements on those they replace: many other alterations, however, such as those of the preparations of phosphorus, are not less unsatisfactory than those in the late edition.

Of the substances omitted from the new Pharmacopœia, few will regret the absence of castoreum, ulmi cortex, dulcamara, and rhamni succus; but it is a serious error no longer to recognise as officinal the green iodide of mercury, so largely used in many different countries, and frequently tolerated when the red iodide cannot be borne. The leaves of stramonium also were valuable for smoking purposes in spasmodic asthma; and even areca was occasionally of use in cases of tapeworm, where male-fern had failed.

Of the 114 new remedies or preparations added, upon most it is simply setting the seal of official sanction, as they have been already brought into familiar use in daily practice. Lamellæ, or "discs," are now officinal, prepared with cocaine, physostigmine, and atropine; and "tabellæ nitro-glycerini" are also introduced: these are defined as tablets of chocolate, containing $\frac{1}{100}$ grain of pure nitro-glycerine. These tablets, we must suppose, fall from heaven (like manna), all ready for use; for the editors do not condescend to tell us what either nitro-glycerine or chocolate is, or how they can be recognised, or how these tablets are to be made, or what is to be done with them when they are thus miraculously obtained!

Upon a few of the individual additions we must make a brief remark. Apomorphia is introduced in the form of the hydrochlorate; and a solution for hypodermic injection is given, which "must be made as required for use:" this is very inconvenient, and would be obviated by having lamella of this alkaloid also. The use of apomorphia, except by the hypodermic method, is not alluded to, nor any dose stated, although, given by the mouth, it is a valuable expectorant in cases where the sputum is viscid. Cocaine is presented to

us in the form of lamella only, although it is desirable to have an officinal solution of, say, 4 per cent. strength. Under the elegant Latinity of ext. cascaræ sagradæ, a preparation of the rhamnus purshianus is now recognised, as well as similar preparations of the rhamnus frangula. As these remedies are now in constant use, we think this is a step in the right direction.

The introduction of the active principle elaterin, in place of the uncertain substance elaterium, in the composition of the compound powder of the drug, is also a right step; but the doses given of elaterin and of pulv. elaterini co. do not tally with one another. Jaborandi, and its alkaloid, pilocarpine, in the form of the nitrate, are placed in the new Pharmacopœia. The galenical preparations of jaborandi seem uncertain in their action; perhaps on account of the presence of the countervailing alkaloid, jaborine, which exists also in varying quantities in the leaves of the plant. Pilocarpine is, therefore, much to be preferred, and is usually administered by hypodermic injection. The editors do not specify whether the doses which they recommend are intended to be given by the mouth or otherwise: the maximum dose mentioned ($\frac{1}{2}$ grain) is certainly too large to be safely administered by the skin. Seeing how frequently ext. belladonnæ is prescribed for internal use, the introduction of another extract without a very distinctive name is likely to prove another source of dangerous mistakes. The large development of antiseptics since the publication of the previous edition of the *British Pharmacopœia* is fairly recognised by the introduction of several fresh antiseptic substances. Boric, salicylic, and chromic acids, iodoform, menthol, thymol, eucalyptus oil, and the sulpho-carbolates of zinc and sodium, are now added, and most of them rightly so. Two new remedies are introduced, of which nothing but disapprobation can be expressed: actæa racemosa, now called cimicifuga, which experience long ago showed to be completely useless; and staphisagria, which, though certainly a potent destroyer of pediculi, is not more so than other well-known substances, and is a violent and dangerous poison. In the preface to the present edition it is stated that "pains have been taken to bring the whole of the matter up to the existing state of knowledge;" but if that is so, why are we not provided with certain remedies of more value, and in more general use, than cimicifuga, for example? Why are not euonymin, convallarin, hamamelis, and homatropin, *inter alia*, placed in the new Pharmacopœia? Of the newer anti-pyretics, kairin, anti-pyrin, thallin, and such like, we must presume the editorial committee have not yet heard,

though they are, and will continue to be, largely used, M. Jacoud's statements notwithstanding. In conclusion, it is to be regretted that, having plagiarised the Pharmacopœia of the United States so largely, the General Medical Council should not have had the courtesy to acknowledge the source from which nearly all their valuable innovations have been derived.

A Guide to the New Pharmacopœia. By PROSSER JAMES, M.D., Lecturer on Materia Medica at the London Hospital, &c. London: J. & A. CHURCHILL, New Burlington Street. 1885.

The publication of the new edition of the *British Pharmacopœia* renders it incumbent upon all practitioners and medical students, as well as dispensing chemists, to make themselves acquainted with the differences between the *British Pharmacopœia* of 1885 and its predecessor. These differences are numerous—dispersed throughout the whole work,—and their complete realisation, therefore, a tedious and complicated task. Dr. Prosser James has early come to the rescue of the bewildered investigator, and, Ariadne-like, provides a clue, by which the therapeutical Theseus may be guided through the pharmaceutical labyrinth.

While Dr. P. James has given a synoptical account of the alterations, introductions and omissions which have been made in the new volume, he has not confined himself to mere bald statements, but in most instances has taken occasion, when speaking of a remedy, to give his own experience of its use, and to criticise and comment upon the preparations of it, which may be in the new Pharmacopœia.

We shall, doubtless, soon be having new editions of many of the existing text-books on Materia Medica, revised so as to bring them *en rapport* with the new Pharmacopœia. Pending the publication of such, or to those persons who wish to make the old text-books still continue to serve their purpose, we cordially commend this book. Though published so soon after the other work, upon which it is a commentary, it bears evidence of close study and analysis of subject-matter, and forms a compendious, comprehensive, and critical *vade-mecum* to the alterations in that "inextricabilis error," the *British Pharmacopœia*, 1885.

An Index of Surgery. By C. B. KEETLEY. Third Edition. Pp. 506. London: SMITH, ELDER, & Co. 1885.

We have already had occasion favourably to notice this work, and the early call for a third edition does not surprise us. The author is wise in eliminating, as well as in adding, and we congratulate him on his success in both directions.

In these busy times there is obtruded on us the uncomfortable question: Is the English language too prolix? Is stenography as feasible in composition as in penmanship? Are verbs and pronouns and adjectives necessities or luxuries in writing? When Mr. Keetley succeeds in conveying in a dozen words the meaning that others would spread over half a dozen sentences, we cannot help doubting the full value of the usual estimates put upon English grammar and rhetoric. It looks as if facts were too abundant and language too much drawn out for the modern student of medicine. In the interests of cerebration and of language we could wish that neither position was tenable: we are afraid that both positions are assured.

Contributions to Pathology and the Practice of Medicine. By J. R. WARDELL, M.D. Pp. 807. London: H. R. LEWIS. 1885.

By the advice of friends who stand high in the profession, Dr. Wardell has collected in this large volume the sum of his contributions to scientific medicine. Many of the papers are classics, and have long passed beyond the criticism of the reviewer: among these, we would specially call attention to the article on "Relapsing Fever," which still remains the best in existence. Of the others—and they are very numerous and very varied—nearly all have appeared at different periods in journals or other medical publications. Some of these are of a very high order of excellence; there is not one that is commonplace or mediocre.

Dr. Wardell's death has followed quickly on the fruition of his labours. He leaves behind him a valuable legacy in this work, the result of many years of quiet, honest labour, the outcome of a scientific and cultured mind.

A System of Obstetric Medicine and Surgery. By ROBERT BARNES and FANCOURT BARNES. Two Vols. Pp. 592 and 738. London: SMITH, ELDER & Co. 1885.

To seek to estimate this work by any standard lower than the highest, would be unjust to its authors. It is mainly the production of a man widely and honourably known as of high repute in his department. Others have assisted him: his son, chiefly in the sections devoted to operation; Mr. Noble Smith, in the malformations and diseases of the embryo; and Professor Milnes Marshall is entirely responsible for the section on embryology. This subdivision of labour is not without its significance. We do not for a moment question its value; but we confess to a feeling of something akin to trepidation when we face the fact that a teacher and specialist in obstetrics, holding the position of Dr. Barnes, does not feel himself competent to guide us over the whole of his selected field of study and practice. If every future writer is to be as honest as Dr. Barnes in this respect, we fear that handbooks will be displaced by systems, and systems become expanded into encyclopædias. In the meantime, what of the student and our common three score years and ten?

That the work fully and adequately represents the modern science and art of obstetrics goes almost without saying. Little more is left to the reviewer than to try to gauge the degree of its excellence. We confess to being disappointed with the engravings, not with their artistic character, for that is everywhere unimpeachable, but with their selection. Surely the sectional drawing of the female pelvic organs (vol. i., p. 20), after Sappey, has been abundantly proved to be erroneous. And many, too many, of the others have been produced and reproduced almost *ad nauseam* in works which have been before the profession for years. It may be hypercritical to take exception to this hereditary transmission of engravings; but we think a writer with the capacities and opportunities of Robert Barnes ought, like Thackeray, to make an endeavour to illuminate his own work with his own candles. The only other depreciatory remark we have to make refers to the index: it is not bad, but it might easily be better. And we hope the author will see his way in a future edition—which will, doubtless, soon be called for—to provide a general index for the whole work.

Returning to the excellences of the work, we think special commendation is merited for completeness, balance, arrange-

ment, and language. Nothing of importance in obstetrics is omitted; much that receives little or no mention in other works is included; and a just and fair proportion of space, without personal predilection or bias, is given to each subject. Each matter is placed in its proper position, and adequately treated; there is little or no overlapping; all is clear, definite, and straightforward. Of great value are the frequent summaries and conclusions placed, in well formulated sentences, at the end of many of the disquisitions. Most of all would we commend the language. It is no small boon to the jaded intellect, in these bustling times, to be able to travel in easy conveyances over well-paved roads; and we heartily thank Dr. Barnes for the comfort with which he has carried our mind right through to the end. His clean-cut, clear, and elegant sentences are a positive pleasure to read. One who writes so must think so: it is a strong presumption in favour of the excellence of a work that its author knows how to speak out with clearness and definiteness, as well as elegance and vigour. We are willing to believe that our readers will find the presumptive virtues of Dr. Barnes's work more than borne out by its perusal.

Notes of Medical Experiences in India, principally with reference to Diseases of the Eye. By S. E. MAUNSELL. Pp. 115. London: H. K. LEWIS. 1885.

This is a modest record of work of a high order, done under disadvantageous circumstances. That the writer, without skilled assistance of any sort, and under difficulties that would have disheartened most men, was able, in eleven working months, to perform 267 extractions of cataract among the natives of India, and have only 22 failures from all causes, is very geatly to his credit. His remarks are not entirely surgical, or even medical: personal experiences of travel and residence in regions little known help to fill the pages of a very pleasing and interesting little volume.

The Saline Waters of Leamington. By F. W. SMITH, M.D. Second Edition. Pp. 96. London: H. K. LEWIS. 1885.

The second edition of this little work, which we have already favourably commented upon, is enriched by the

insertion of three separate analyses of the Leamington waters by distinguished chemists, and by other additions which will increase its value. It is a handy, unassuming, and trustworthy guide to one of the most popular of our English spas.

A Practical Treatise on Diseases of the Kidneys. By C. H. RALFE. Pp. 572. London: H. K. LEWIS. 1885.

We have formed a high opinion of this work. For the ordinary purposes of practitioner and student, we consider it about the best we possess. Perhaps more facts and more philosophy may be found in such works as those of Roberts and Dickinson, but we doubt if a more accurate concrete knowledge of Diseases of the Kidneys could be acquired from these large works than from this small one. That Dr. Ralfe shows a full and perfect acquaintance with his subject, practical as well as bibliographical, is abundantly evident. The arrangement of the work is enough to show this. It is easy enough to write continuously and discursively our individual thoughts and observations on disease: if our thinking is sound, and our observation is exact, our writings may be suggestive and instructive. But, to be of real educational value to the ignorant student, a written work must be arranged according to scientific methods—specific, generic, etiological, and practical. In this sense, Dr. Ralfe's work is a genuine success. Beyond this, criticism need not go. On special points, studied by special men, it is just possible that our author may not give full satisfaction. It is clear, however, that on broad principles he has produced, within reasonable compass, a monograph which, for accuracy and extent of information conveyed, it would be difficult to rival.

Unconscious Memory in Disease. By CHARLES CREIGHTON, M.D. Pp. 212. London: H. K. LEWIS. 1885.

This is undoubtedly a powerful and remarkable book. We were fully prepared for abundant evidences of philosophic breadth and culture in anything that falls from the pen of Dr. Creighton relating to pathology; but we confess we were somewhat surprised to find proofs of at least equal capacity in dealing with matters relating to practical medicine. No

doubt pathology is the *scientia scientiarum* for all medicine; a profound knowledge of pathology renders all medicine easy. But, in this country at least, we have become so familiar with the mere mechanician, the cutter of sections and the mounter of slides, as the only exponent of pathology, that we have some difficulty in realising the fact that a learned pathologist may be at the same time a high-class physician.

It is impossible to give, in the space at our disposal, an adequate idea of the purpose and aims of Dr. Creighton's work. It is mainly an estimate of the influence of habit and memory, physiologically, pathologically, and practically considered, as continuing disease. His thesis is supported by an abundance of illustration, drawn from the most varied sources, and the propositions are knit together by conclusions drawn from practice. If we were surprised at the number of facts which the author has been able to concentrate on his subject, we were charmed with the manner in which he has manipulated them, and logically evolved their meaning. Even if we were prepared to dispute any of his conclusions, we could not fail to admit the masterly handling of logical methods whereby he reaches them. In language Dr. Creighton has here excelled himself. Possibly this is the first opportunity which his practical labours have afforded him of showing his powers in exposition; certainly this opportunity has proved that Dr. Creighton is second to no living medical author as a writer of the English language.

We confess to having laid down the work with something like a glow of enthusiasm in our somewhat nauseated intellect. It has been the case with one other modern work besides this, that the reviewer could read nothing else till he had finished it—Hilton's *Rest and Pain*. With Hilton's classical work it has little in common beyond this, that it deals with one dominant idea, and that it illustrates and expands this idea with a fertility and breadth that had never before been excelled, or even attempted. We would even go so far as to say that in logic and language Creighton has surpassed Hilton. Our readers will judge for themselves.

Medical Extracts.

A Case of Impacted Rhinolith.

Chiari relates the details of the case of a lady who was sent to him by Nothnagel, and who had been suffering for ten years from inability to breathe through the right nostril, from which, also, there had been a constant flow of purulent matter during the same length of time. Anterior rhinoscopy showed that the mucous membrane of the inferior turbinated bone was so much swollen that it was impossible to see beyond its anterior extremity. On using the sound, a hard, uneven body was felt, which, with a little manipulation, was brought into view. This body was removed by forceps, piecemeal, without much pain. The centre of it was a metal button, on which there was deposited carbonate and phosphate of lime, together with organic substances. The patient, after the operation, complained that too much air passed through the nostril that had been affected, and that the throat soon felt dry; and on comparing the sizes of the inferior turbinated bones of the two nostrils, that of the affected side was much smaller, and so the nostril was larger than the other.

About 40 such cases have been published, and in the large majority some foreign body has formed a nucleus, on which deposit of salts has taken place.—*Deutsche Med. Zeitung*, Nov. 5th, 1885.

Treatment of Whooping Cough by Resorcin.

Dr. Moncorvo, believing that pertussis is caused by parasitic agency, has tried painting the glottis with a 1 to 2 per cent. solution of chemically pure resorcin, and is much pleased with the result; but, as he remarks, the application with the brush is often resisted by the patient. (Anyone who has worked much with the laryngoscope amongst adults, and who knows the difficulty which is often met with in accurately painting the glottis, especially where there is a tendency to spasmodic cough, will appreciate this remark, especially seeing that the majority of patients who suffer from whooping cough are children.—Trans.)—*Deutsche Med. Zeit.*, Oct. 29th, 1885.

The Use of Cocaine in Whooping Cough.

Dr. L. Barbillion, starting with the idea that the paroxysms of whooping cough are produced by irritation of the throat, and knowing, as we do, that cocaine is a very powerful anæsthetic for mucous membranes, has used the drug, with the following results: (1) A lessening of the number of attacks of coughing per diem. (2) Prevention of vomiting during the paroxysm, and of the debility produced thereby. (3) A greater power of resistance against the evil effects of a long and debilitating illness. No ill effects have been noted.

Seven cases are published *in extenso*, of which there were five cures: two cases were fatal, from broncho-pneumonia complicating the pertussis; one of these had measles also.

The drug was used in the form of a 5 per cent. solution in water, and the posterior pharyngeal wall was painted two to four times daily. (The cost of cocaine being now only about 4/- a gramme, the treatment has no disadvantage in point of costliness, and is certainly logical, and worthy of a trial.—Trans.)

Contribution to the Question of a Mechanical Treatment of Whooping Cough.

Dr. Goldschmidt puts forth the hypothesis that in pertussis we have to do with an affection of the nose and the naso-pharyngeal cavity, and that the paroxysms of coughing are due to mucus entering the larynx, as the result of aspiration, or simply of gravity. The treatment to be adopted is to use an antiseptic nasal wash every two hours; and the author says that he has had good results.—*Rev. Mensuelle Laryngologie*, &c., October, 1885.

On the Treatment of Catarrhal Phthisis, of Hæmoptysis, and of Chronic Bronchitis, by Terpine.

Germain Sée gives the following *résumé* of his paper on the "Treatment of Catarrhal Phthisis, of Hæmoptysis, and of Chronic Bronchitis, by Terpine:"

1. It diminishes and quickly arrests purulent expectoration in catarrhal forms of phthisis. Whether the muco-purulent secretions proceed from the bronchi, irritated by tubercles, or from the walls of pulmonary cavities; whether the malady is at an early stage, or at a phase of purulent breaking down, or even of cavities already formed; terpine should be used whenever the formation of pus is sufficiently abundant to tire the patient, to exhaust the strength, or to cause him to waste away.

2. It should be used with success in the hæmoptysis of

the early stages of tuberculosis; that is to say, when the disease has not yet developed large cavities, with aneurisms of the pulmonary arteries.

3. In the treatment of pulmonary catarrhs; of chronic bronchitis not dependent on asthma, and only producing dyspnœa by choking the bronchi, terpine constitutes the best method of lessening bronchial hypersecretion.

4. The action is quick, sure, and free from physiological inconveniences, rendering it preferable to preparations of syrups of turpentine or tar, or of shoots of pine, which contain so little of it; and to essence of turpentine, which is not tolerated. It even offers advantages over creosote, on account of its perfect innocuity and easy digestion.

5. The best way of administering this medicine is either in the form of pills or tincture, and the best dose is one gramme.

6. In catarrhal, or emphysematous, or nervous asthma, which is to be distinguished from primary catarrh, iodine and pyridine have an incontestable superiority.—*Bulletin de l'Académie de Médicine*, No. 30, 1885.

On the Treatment of Obesity.

Germain Sée, on "The Treatment of Obesity:"

1. A physiological regimen contains from 120 to 130 grammes of nitrogenous matter, obtained from 250 to 300 grammes of muscular flesh, or albuminates; of 100 to 120 gr. of neutral fats, plus 250 gr. of carbo-hydrates, supplied by 300 to 400 grammes of starch or sugar. These proportions should be modified so that the musculo-albuminous substances do not materially surpass the normal quantity; for meat taken in excess would of itself make fat. Fatty matters, easy of digestion, could, without inconvenience, be given in quantity amounting to from 60 to 90 gr.; the carbo-hydrates should be reduced to a minimum; as to vegetable food, it contains nothing nutritive.

2. Fluids, far from being suppressed, should be increased, to facilitate stomach digestion, and encourage general nutrition; but alcoholic drinks should be suppressed, especially ale; also mineral waters, as a rule: these should all be replaced by coffee and infusion of tea (as warm as possible).

3. Muscular exercises of all kinds are necessary to the obese; riding, which is a passive exercise, being excepted.

4. Vapour baths, warm baths, and, above all, hydropathy, offer some advantages.

5. Amongst medicines, the most useful are very small doses of iodides; chloride of sodium waters act only temporarily. Alkaline preparations and waters, especially good for

fat diabetics, have no precise action on common obesity. All other medication is, to say the least, useless.—*Bulletin de l'Académie de Médicine*, No. 40, 1885.

Cholera.

The following are the conclusions on "Cholera," presented to the French Academy of Medicine by its Commission:

1. In the regions of France from which we have received answers from medical men, the cholera, as a rule, has only appeared as coming from a country already contaminated. For, in three-quarters of cases, this importation has been recognised as such; and for the other quarter, the importation is more than probable, for reasons set forth in the report.

2. If we trust entirely to observations contained in these reports, we see that cholera developes with less intensity in populous centres than in small localities. It is, therefore, a grievous error that the inhabitants of towns should, in time of epidemics, fly to the country.

3. A general want of cleanliness, and, above all, the bad habit of throwing human evacuations anywhere, is the principal cause of the propagation of the disease. For, in time of cholera, the dejections of a patient presenting only slight diarrhœa may contain cholera germs of a serious nature.

4. The germs of cholera are often spread by water contaminated by dejections of a patient, and it is generally in drinking this water that the disease is contracted.

5. The storms, that so often precede or aggravate cholera epidemics, act in contaminating the water by disseminating into them any impurities that may be lying about the soil.

6. It is because drinking waters are ordinarily well trapped and preserved from contaminating matter, that towns give more resistance to the spread of cholera. Nevertheless, a few towns supplied by river water lose this privilege.

7. In all cholera-haunted localities, however, low-lying districts situated near rivers, and places where the purity of the water is doubtful, are the most dangerous to inhabit.

8. Houses inhabited by cholera patients, as well as all dejections, clothes, or any other objects, should be thoroughly disinfected, according to the method prescribed by the Consulting Committee of Hygiene. Such precautions seem to have often arrested the spread of cholera at its beginning. But, to be entirely efficacious, medical men should exert great vigilance in prescribing disinfection; for the ignoring of early cases of cholera, or even of slight choleraic affections, may give rise to contamination of water, and propagation of the disease.

9. The chances of contracting cholera are greatly increased by old age, exhaustion, infancy (early), as well as by alcoholism and by general and personal want of cleanliness.

10. A first attack of cholera offers no immunity, even for a short time, against a second attack, inasmuch as numerous relapses have occurred even during a short epidemic.—*Bulletin de l'Académie de Médicine*, No. 35, 1885.

Obituary.

WILLIAM MICHELL CLARKE, M.R.C.S., L.S.A.,

Consulting Surgeon to the Bristol General Hospital.

We have to record with much regret the sudden and unexpected death of William Michell Clarke. The deceased gentleman died at his residence, 2 York Buildings, Clifton, on October 2nd. He had been somewhat indisposed for a few days, but his friends noticed nothing in his condition to excite alarm; indeed, he continued his professional work to the very last, and saw patients on the day preceding his death. Never of very robust strength, he was able by careful management to carry on one of the largest practices in Clifton and its neighbourhood, and his loss will be greatly felt by numerous patients and friends. He leaves a void in professional circles that will not be easily filled, and by none will his loss be more felt than by the members of the Bristol Medico-Chirurgical Society. Mr. Clarke was, in 1879, President of this Society, and read a most interesting address on "The Natural History of Disease." He was also a frequent contributor to the ordinary meetings, besides taking a deep interest in the general well-being of the Society.

Mr. Clarke may be looked upon as one of our most successful practitioners, and he owed his success entirely to his own individual efforts, and to thorough and conscientious work. His example may be taken as showing what may be accomplished in our profession without extraneous aid by a man who is thoroughly in earnest.

Mr. Clarke began his professional career by being apprenticed to a country Surgeon at Bodmin, his native town. He afterwards entered St. Bartholomew's Hospital, where he was known as a most industrious and painstaking student. After leaving St. Bartholomew's, he was appointed House Surgeon to the Bristol General Hospital, which at that time

contained only about twenty beds, and was located in two or three ill-contrived houses. Here Mr. Clarke worked hard and well, and ultimately had the satisfaction of being Surgeon, and afterwards Consulting Surgeon, in the same Institution, when it had a local habitation worthy of its name. As a Surgeon, Mr. Clarke was most careful and discriminating, and had performed most of the great operations in Surgery; and although he was compelled by the rapid increase of his private practice to resign the active duties of Surgeon at the end of the comparatively short period of ten years, yet he continued his connection with the Hospital in the capacity of Consulting Surgeon, and was most regular in his attendance on the fixed operating days; his services as Consulting Surgeon being much appreciated by his acting colleagues.

In his private practice, which was rather more medical than surgical, he was successful more than ordinary men. Beginning practice first near the Hospital, he speedily moved up to Clifton, where the public and his medical friends soon recognized his merits; and especially was he favourably known to the late Drs. Symonds and Budd. A few years after settling in Clifton, Mr. Godfrey, who was one of his surgical colleagues at the Hospital, contracted typhoid fever and died; and Mr. Clarke succeeding to his practice, found himself, at a comparatively early age, one of the busiest men in Clifton.

Mr. Clarke had not only filled the office of President of the Bristol Medico-Chirurgical Society, but had also been President of the Bath and Bristol Branch of the British Medical Association. He was also formerly Lecturer on Medical Jurisprudence at the Bristol Medical School, and at the time of his death was one of the Governing Body of that Institution.

Few men were more unselfish with regard to acquiring patients; and in all his dealings with them, and also with the members of his own profession, he was honest, honourable, and straightforward, and was in consequence held in the highest esteem, as was testified (if such testimony were needed) by the large number of professional friends who attended his funeral.

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